thing which he has at his finger's end to make the distinction between the *necessary* and the *sufficient* condition for the truth of a statement, and there is no reason why other scientists should not speak with the same precision. One thing is the *necessary* condition for the truth of another, if the latter cannot be true in its absence; it is the sufficient condition, if it must be true in its presence. It may be matter of question whether 'test of truth' should be used in the sense of necessary or of sufficient condition of truth, but it certainly should not be used in both senses in the same sentence. 'Evidence' is the *sufficient* condition for the truth of a statement, but it is not in every instance *necessary*. I need no evidence to convince me that I am conscious. Now those who regard conceivability in the way that Prof. Brooks objects to, do not for a moment consider it to be a *sufficient* condition of the truth of any statement, but they do consider it to be the *necessary* condition of the truth of every statement. The inconceivability of a statement is for them the sufficient test of its falsity, and its conceivability is the necessary test of its truth. Instead of saying, therefore, with Prof. Brooks, that *the test of truth is evidence and not conceivability* (a statement which gives me a slight feeling of dizziness), it would be better to say that *the test of truth is evidence, and inconceivability is no criterion* (or test) *of falsity*, provided the exact terms, necessary and sufficient, should be considered too pedantic.

I have used the terms *necessary* and *sufficient* because they have been consecrated to this purpose by the mathematician, but I believe that *essential* and *sufficient*, or perhaps *requisite* and *sufficient*, would convey the meaning much better for ordinary language. We should then say, *evidence is a sufficient test* and conceivability is not a requisite test of truth.* The sentence "conceivability is not a necessary test of truth" is somewhat ambiguous; it might mean 'is not a test such that the truth necessarily follows from it,' instead of 'is not a test which it is necessary to have fulfilled if the truth is to hold.' But 'requisite test of truth' is not open to any ambiguity.

* That, for nearly all truths, evidence is also a requisite test, is true, but is denied by no one.

I am convinced that if the terms requisite and sufficient (or something equivalent to them) were to come into common use as defining the *kind* of ground, reason, argument, condition or test that the writer has in view, it would conduce very much to facility of comprehension on the part of the reader. M. M.

THE TEMPERATURE OF THE EARTH'S CRUST.

Mr. Sereno E. Bishop, in his letter in Science, March 13th, remarks that it would be interesting to ascertain what are the rates of increase of temperature now under regions where the subsoil is permanently frozen, as in the tundras of Siberia and Alaska.

Attention may here be called to the Report made to the British Association in 1886, by the committee appointed to organize a systematic investigation of the depth of the permanently frozen soil in the polar regions. Of some twenty-two localities mentioned in that Report, Jakutsk, Siberia, lat. 62°, is perhaps the most noteworthy, the limit of the frozen soil being 620 feet and the temperature rate 1° for 28 feet.

The transcendental formula employed by Lord Kelvin in his well-known chapter on the 'Cooling of the Earth' furnishes results in marked harmony with the temperature rate as determined by many observations. (Prestwich, Proceedings of the Royal Society, 1886.) It does not logically follow, of course, that Lord Kelvin's premises are necessarily correct. However, whether we accept the argument in the 'Cooling of the Earth' or rely on observations alone, we must for the present regard 1° F. per 50 feet (approximately) as expressing the law of the rate of increase of the temperature of the earth's crust near the surface; some local factor should be looked for as the cause of such an exceptionally low rate of increase as that found in the Calumet mine, or such a high rate as that in the Jakutsk mine. In any case it is scarcely safe to assume, as Professor Agassiz seems to do, that the rate observed to the bottom of the Calumet mine holds to the depth of 19 miles and beyond, and thence to conclude that the earth's crust has a thickness of 80 miles. The crust of the Lake Superior region may have counterbalancing abnormal features, so that the low temperature rate for the first mile is amply

atoned for before Lord Kelvin's 100,000 feet level is reached.

As regards Mr. Bishop's ice-cap hypothesis, would not an ice cap, on account of the low conductivity of ice, have the effect of raising the temperature rate instead of lowering it?

ELLEN HAYES.

WELLESLEY, MASS., March 18th.

THE PREROGATIVES OF A STATE GEOLOGIST.

EDITOR OF SCIENCE: As is well known to many of the readers of SCIENCE, the writer of this note spent the greater part of five summers in Missouri, studying the crystalline rocks and associated formations over an area about seventy miles square in the vicinity of Pilot Knob, and has published a number of papers concerning them. While Winslow was State Geologist I published the first half of Bulletin 5, and sent in manuscripts to accompany the Iron Mountain sheet, the Mine la Motte sheet, and my final report, which was to constitute a monograph, the last manuscript leaving my hands in August, 1893. The Iron Mountain sheet was engraved and proof sent me for my final revision of the geological boundaries, as was also the proof of my part of the accompanying text, before Winslow left the position of State Geologist, while as early as March, 1892, the Mine la Motte sheet was drawn and I marked the geological boundaries on it, although it has not yet been published.

Shortly after assuming control of the State Survey Office Dr. Keyes wrote me that he would soon take up the manuscript of my final report. On September 23, 1894, he wrote me as follows:

"Since looking over your MS. rather carefully I have come to the conclusion that it would be best perhaps for me to write an introductory chapter on the general geology of the region. We have now so much new material on hand in this direction, and the topographical sheets and reports on this have been completed this summer and are now ready for the printer, so that it would greatly enhance the value of the report to incorporate this work. So much more also is known in regard to the Cambrian since I have made a trip into the region. * * * I will revise the I. and II. chapters, if you are willing, *so as the introductory will not cover the same ground*; so you need not give these chapters much attention." (Italics are mine.)

Knowing the facts regarding the preparation of the sheets as above stated, it is difficult to understand how so much 'new material' could have been gathered in so short a time.

I wrote him in substance in reply to his letter of September 23, 1894, that of course he could write any introductory matter he chose, but that I very much hoped he would not borrow too freely from my manuscript in so doing. On January 29, 1895, he again wrote me:

"Regarding the other part of your letter I can assure you that I do not wish to detract one iota from the work or to deprive you of any credit on account of changes which may be made. Before it is printed I will talk or perhaps 'write' the matter over with you."

The manuscript was finally sent me as Dr. Keyes had revised it, but my first two chapters had been so changed and so many positive errors introduced that I wrote the State Geologist it never would do to have it published in that form. The result was he visited me in April, 1895, and we talked the matters over freely, as I thought. He consented to every change I suggested excepting that he wished my original manuscript abridged more than I desired. During this conversation not a word was said or even intimated that the chapter on the general physiography was not mine. I told him certain of the geological discussions which he had introduced were so different from what I had written that I did not care to be responsible for them. But I never thought of this being his introductory chapter, as he said nothing about it, and as his name was not attached to it, although he called this the first proof. No further word on the subject was sent me, and I was given no chance to further read the proof, although only twelve hours from him by mail. On November 1, 1895, I received the publication which appeared as a part of Volume VIII. of the Missouri Geological Survey. Much to my surprise I found that the whole of the physiographic descriptions and much other matter which I thought was entirely mine appeared under his name without any intimation